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# Fraud vs. Error: The Dingelling of Science

By BERNARD D. DAVIS

The recent interest of Congress in fraud in science is justified, because in a number of serious cases our academic institutions have responded very badly. We scientists have an even stronger interest in seeing this problem handled better, for we build our whole enterprise on a foundation of communal trust. Unfortunately, the congressional inquiry has become a crusade, punitively pursuing one of the most distinguished and productive biomedical scientists, David Baltimore of the Massachusetts Institute of Technology.

Last spring Rep. John Dingell (D., Mich.) held a subcommittee hearing on fraud in science, with the stated goal of preventing waste of tax money. He heard not only about several real cases, but also about a very shaky one. A postdoctoral fellow who had resigned (and quit science) questioned the veracity of the data in a paper written by her mentor in collaboration with Mr. Baltimore. This paper, on the effect of introducing a gene for forming a specific antibody into the fertilized eggs of mice, was exceptionally complex and also exceptionally interesting, for it concluded that in these "transgenic" animals the introduced gene and the host's genes did not act independently but interacted in altogether unexpected ways.

The congressional hearing had several serious faults: It blurred the crucial boundary between fraud and normal error;

the dispute was far too technical for such a forum; and there was no opportunity for the accused to rebut the charges by the aggrieved witness. Major scientific organizations registered protests. But Mr. Dingell had hooked a big fish, with a Nobel Prize.

The National Institutes of Health (NIH) set up an outside committee of experts to examine the issue. Its report cleared the authors of any charge of misconduct, found errors of varying magnitude, and concluded that these did not undermine the main conclusions of the paper. As is usual, the draft report was submitted to the interested parties for correction and clarification. It led the authors to publish a retraction of the errors that they considered most significant.

Mr. Dingell might have been pleased at this outcome, and might even have expressed regret for the earlier implication of fraud. But his response was vindictive: a furious letter, protesting that the NIH had "defused" his issue by stimulating a premature retraction, and demanding that the inspector general of the governing department investigate possible criminal actions.

In addition, while Mr. Dingell could no longer hold his big fish on the suspicion of fraud, in a recent report of his subcommittee he presents a new goal: not to save tax money, or even to investigate misconduct (a broader term that has replaced

"fraud" in most discussion), but "to ensure the continuing pre-eminence of American science." The door is thus opened for well-intentioned but clumsy interference of unlimited scope—abandoning the centuries-old recognition that science flourishes best if scientists are given broad latitude in ordering their own affairs.

Specifically, Congress has now suggested that an Office of Scientific Integrity be established; and that it audit scientific records on a random basis. But such auditing could rarely detect falsification in scientific records, which are very different from financial records. Moreover, in science nature is the ultimate auditor, and so the level of fraud is unusually low; hence random scrutiny would not be cost-effective.

If, on the other hand, the auditing sought errors, it would be useless: Inconsistencies in raw scientific data (as in the Baltimore case) are inevitable and require judgment by the investigator; hence science has developed well-tested methods for either correcting important errors or bypassing minor ones, ultimately building a solid edifice. Above all, the atmosphere created by auditing would erode the morale of scientists, discourage the recruitment of bright young students, and encourage institutions to overreact.

Such overreactions are already at hand—for example, the dean of Harvard Medical School dismissed a distinguished

tenured professor for plagiarism though it was only the minor form of this offense, copying paragraphs in a review without rewording, rather than the serious form that falsely claims credit for discovery. In the Baltimore case, with no base of misconduct, we find another form of overreaction. The NIH committee criticized the authors for retracting only part of the uncovered errors, and the NIH administration now requires them to publish a retraction of all the others, including even clerical ones.

This decision is regrettable. It deprives a distinguished scientist of his right to exert judgment about what is a significant error, and to have the outcome of a dispute determined in the traditional way by further work, rather than by legal pressure.

Moreover, the NIH documents made no effort to focus on the broader implications of this degree of interference for the future atmosphere and style of scientific research. One can see why the NIH would take this position, for otherwise one can imagine the headline, "NIH Condone Error in Science." But the effort to avoid it has given an unfortunate message. It encourages legislators to delve into affairs where they cannot be helpful. And it encourages scientists to do more pedestrian work, rather than face the increased risk of mistakes when they explore challenging problems and push methods to their limits.

The attack on the Baltimore paper has already cost a great deal of money, has damaged the image of science on the flimsiest of grounds, and has been a tragedy for most of the participants. Moreover, Mr. Dingell has clearly intimidated scientists into dealing with the problem within his frame of reference, rather than that of the traditions of their profession. How far will this process go? Neither science nor society will benefit from a paralytic legislative crusade for an unattainable degree of purity.

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